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STANFORD UNIVERSITY SCHOOL OF MEDICINE
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Dear Gunther

I enjoyed your biography of DNA in Daedalus. It is a valuable contribution.

My postscript now is probably no news to you, but as you may well become the official historiographer of molecular biology, I thought I should put it on the record to you.

Your use of the 1950 Genetics symposium was brilliant, and I can't quarrel with what you said, except for a rather specialized personal issue. I would not want to be stereotyped (nor would you intend this) as an intransigent classicist -- you could have used my Nobel lecture of '58-59 to make exactly the same point you were stressing, about the post-classical revolution.

STENT
The issue I faced in 1950 was not so much "gene" versus "DNA molecule", an inference that some might draw from your portrait, but rather whether the genetics of bacteria could be homologized with that of other organisms, in effect, whether molecular biology (being developed in microbes and phage) was relevant to classical genetics. Many people were denying this -- note Harriett Taylor's remarks of that era; and in fact it was not until 1951, under the impact of factorial-recombination studies with Salmonella transduction and (for the first time since 1928!) pneumococcus transformation, that such a homology was generally accepted, i.e. that one could put fragment-transfer into the same framework as recombination in higher forms. This was why I struggled (as usual, unsuccessfully) to establish a precise but generic definition for transduction.

In 1950, unfortunately, I had no DNA-chemical data to bring to bear on E. coli genetics, not for lack of interest. And to put it bluntly I was not technically up to the task of exploiting the indirect approaches that became available during the 50's for a molecular analysis of E. coli conjugation. (And as Al Hershey used to say, too many unexpected but exciting discoveries got in the way.) This discouragement was my motive to drop E. coli after 1958, and to turn to E. subtilis where I had more hopes of a direct convergence of chemical and genetic analysis.

I hope you will not overlook my 1958/9 retrospection (which was intended to destroy any residual reservations among classical geneticists about the pertinence of the new wave; and may I say also, to help direct the attention of the Nobel Prize Committee to Watson-Crickology) as a more comprehensive ideological statement than the 1950 'last gasp'.

I hope we'll be able to see you again soon. All our best,

Joshua